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BOOK REVIEWS

METHODS AND PRINCIPLES

Primitive Man. (Proceedings of the British Academy, vol. VII.) G. ELLIOT SMITH, F.R.S. London: Published for the British Academy by Humphrey Milford, Oxford University Press, 1917. 50 pp. 3 s. 6 d. net.

This comprehensive and suggestive paper is a timely plea for the study of man's history as a closely interrelated whole and in fact a direct claim also for the essential unity of civilization (p. 2). It is the first of the kind in recent years—at least in the English language—which has come to the reviewer's attention and while it expresses quite forcibly several ideas which he has himself hitherto lacked the courage to put forth, it also oversteps what seem to him to be the reasonable bounds.

There was a time, not long past, when anthropologists had an argument over the singular or plural origin of the human species and which ended, it seems, in a victory for the monogenists. We now appear to be entering upon a discussion of the unitary or multiple origin of cultural traits and in view of the suggested analogy perhaps we ought to yield the point at once. For Prof. Smith, if I have understood him correctly, maintains that a given culture trait or complex of traits arose at a particular time in a particular place—to be specific, in the vicinity of Egypt—whence it was transmitted by simple diffusion sometimes to the ends of the earth (see pp. 28, 29, 33 and 38). At first this seems a most illuminating flash but when the actual facts of trait distribution over the world are examined in its light many difficulties arise.

Let us agree for argument's sake that all the primary inventions originated somewhere near the meeting place of Europe, Asia and Africa. Then, in view of the fact that the earth's continental outlines have remained practically undisturbed since before man began his inventive career, we should expect the oldest of his ideas to have the widest general distribution while the latest inventions, other things remaining equal, would have traveled only a short distance from their place of origin. That is, if we may venture to represent the combined time and space relations graphically, we should have something like a pyramid or a cone rising by step-like stages, built up as it were of blocks of successively smaller and smaller dimensions. The foundation block, representing the

Paleolithic culture stage, would be the largest in both dimensions. The Neolithic would be next in size and so on up to the Iron Age, let us say. Differently stated, *i. e.*, considered merely as a spatial phenomenon, we should have a distribution arrangement very similar to that observed in the organic world where a given species normally enjoys only a relatively limited geographic range while its genus, its family, its suborder, etc., enjoy successively larger and larger areas of distribution. In fact Prof. Smith himself (p. 31) expressly recognizes the possibility of some such zonal scheme. Now as a matter of fact we find no such distribution arrangement in the sphere of culture applicable to the world as a whole and yet we do find something of the sort to be true for certain specifically defined subareas as *e. g.*, the American Southwest, where the distribution of several successive styles of pottery conforms in the most beautiful manner to just this idea. Clearly therefore some disturbing elements have entered into the larger general process of diffusion until the facts of cultural trait distribution seem a tangled skein impossible to unravel.

In the first place, as far as contrary evidence is concerned, it appears that the early phases of the Paleolithic culture as such never entered the American continent at all. Either this continent was not inhabited at the time or else our theory of distribution immediately breaks down because our first American immigrants would seem to have entered the New World somewhere on the Solutrean culture horizon, *i. e.*, at the true close of the Paleolithic Age. Our theory of unitary origin may therefore still be intact. But there is a second difficulty not so easily side-stepped. We find in both Middle and South America evidences of two or more somewhat differentiated but relatively highly specialized culture centers and in addition to these distant developments there are several subcenters in the Old World itself, as for example those of the Aegean, the Ganges and the Yang-tse-Kiang basins. That is, viewing the world as a whole, we have not one but several pyramids, pyramids which so far as we can see were not in all cases directly connected with our primary pyramid, except possibly at the extreme base level. The land route from southwestern Asia to Peru is long and difficult and our supposed migrants have left no clearly discernible tracks. There remain to us therefore only two possibilities: either there was a direct oversea communication route connecting southwestern Asia on the one hand with Middle and South America on the other (and it is a very enticing hypothesis) or else our widely separated culture centers were independent developments, the resultants of other factors than those of simple

diffusion by migrating bands. In the third place our supposed primary center—if indeed it was the primary center—long ago virtually dropped out of the race, leaving the subcenters to pursue more or less divergent courses of development; and in our talk of the “rise and fall of empires” we seem tacitly to claim that the different centers have by turn held the dominant position while at the same time the rest as a rule did not simply disappear, except in a political or military sense, but continued their separate courses, having surrendered to the conquerors or rather shared with them only those things which the latter were capable of utilizing.

Now we may admit without hesitation that all of these geographically separated centers have or had many fundamental traits in common, but it is at least equally obvious that they also exhibited traits each peculiarly their own. The ends sought by all these different cultures were more or less the same, but the means of accomplishment differed greatly. Why, it may be asked, should these differences arise if culture was the resultant merely of diffusion by contact? But the answer to that question is immaterial to our purpose at present; we are interested primarily in the trait similarities that exist in certain Old World and New World culture centers where direct contact or, in other words, the process of diffusion can, as it seems to the reviewer, safely be eliminated. What other determining factors are there besides that of diffusion?

When we begin to examine the concrete embodiments of human ideas as they come to us from different times and places we become conscious of a cleavage plane running through the whole mass. Utilitarian objects, as, *e. g.*, tools, at once strike us by their fundamental similarities of shapes; whereas, by contrast, ornamental objects strike us rather by the differences revealed. In order of time, both theoretically and as a matter of demonstrated fact, the utilitarian aspect is the older and more fundamental sphere of expression and, as it seems to the reviewer, it is in a large measure determined by considerations inhering in external nature, while the other aspect, the ornamental, is determined at least in part by considerations inhering in man himself.

If a man wants to punch a hole through a piece of skin or something he requires a sharp-pointed implement, he cannot simply employ anything that happens to be at hand. If he wishes to cut the skin in two parts he needs something with a sharp edge. If he wishes to make a substitute for skin there are only a very few fundamental ways in which textile elements can be united and made to serve the purpose. If he wishes to catch a fish by enticing him to swallow a toothsome morsel at the end of a string he will, to be uniformly successful, need a hook; and

a hook is a hook and not a penholder. If a house-builder wishes to insert a doorway in a masonry wall there are only two or three possible ways in which he can do it: he may bridge the gap in Greek fashion by a horizontal span of wood or stone, he may arch it over as did the Romans or he may draw in the aperture \wedge -wise towards the top, as was commonly done in Middle America. When the American Indian perceived the value and possibility of irrigation he was obliged to tap the neighboring stream above and not below his field, his system of watering could not vary in principle from that of ancient Mesopotamia or of the modern reclamation engineer. And if a civilized man, lost in the wilds without his pocket-knife, should attempt to improvise a substitute from flint he would, to be successful, have to repeat one or more of the primitive methods of flint working. No matter what may be his knowledge and ability, there is no royal road to success, he must return to first principles or else fail. That is to say, there is a conditioning factor in nature which tends to produce like results in different parts of the world. In the commonest of trades and even in the case of so-called unskilled labor there is a "right way" of doing everything and the untutored sooner or later stumble into it, often simply because the "right way," for one thing, is generally the way involving the least effort. But, in reality, it involves more, as any seasoned workman with a spark of feeling for his job will tell us. The "right way" of performing even the most commonplace task is the way which combines the "least effort" with the best (*i. e.*, most useful) and most pleasing (*i. e.*, artistic) result. In other words, in the making of an implement as in the fostering of a culture there is not one but two conditioning factors: there is the limitation of possibilities inherent in nature and then, besides, there is a psychic element; a successful culture is in one sense simply a happy adjustment of those two factors.

But to mention this psychic factor is one thing, for an archaeologist to define it and to describe it is quite another. The old *a priori* argument about the "psychic unity" of the human race is all very well but it gets us nowhere and yet presumably no one will deny that it is there and that it is in some measure responsible for geographically separated identities of cultural traits. The workman's pride in his task well done is not a mere something handed down to him out of his professional environment, it is something given, something which partakes of the character of a hereditary trait. But let us dismiss the subject with an illustration. There is a certain class of people—and the class is not limited by either racial or geographical boundaries—who express a not-

able fondness for bright (we sometimes call them "loud") colors in dress. To all appearances this universal trait is not a cultivated taste originating at some particular place whence it was disseminated over the earth. Rather it appears to have a deeper origin for it commonly persists even in a restraining cultural environment.

In conclusion, then, it seems permissible to state that the earliest and most rudimentary features of culture are largely utilitarian and that they show world-wide similarity not merely because men think alike but because the elementary human requirements are alike and because nature imposes certain conditions or limitations as to the manner of fulfilling these requirements. Accordingly, when we discover that primitive peoples the world over for untold ages have been using the oleocranon bone of the deer or some similar animal for the purpose of making pointed implements, it seems more reasonable to suppose that this happened because this is the particular bone which lends itself to that purpose with the least possible amount of labor rather than that the practice of employing it was disseminated from some particular spot on the globe. When, however, we come to the non-utilitarian culture features, of a somewhat later date in point of origin, the case becomes a little more doubtful. Here "psychic unity" begins to figure; but speaking for myself, I am not claiming everything for it; I am prepared to divide the credit for widely separated cultural similarities more or less evenly between *psychic unity* and *diffusion*. If, for example, Peruvian art were found to embody a whole series of highly specialized Egyptian traits, let us say, then I should be disposed to agree with Prof. Smith that there had been direct intercommunication in relatively late times. But this is not the place to enter upon a discussion of that subject. And so, without categorically denying his theory of diffusion, it seems to the reviewer that neither our facts nor our theories warrant us in wholly ignoring the old arguments about "Environment" and "Psychic Unity." And while I do not venture to demonstrate it, I believe that in the long, world-wide interplay of mind and matter identical situations are bound to have arisen at different times and places so that therefore we may take it for granted that many of our inventions have been repeated not once but several times. To say that two intellects from identical premises could not draw identical conclusions seems equivalent to saying that no one intellect could ever express a sound original judgment which in turn is equivalent to saying that civilization would never have come to be.

But why speculate further on these lines? There is another fundamental point to Prof. Smith's paper the consideration of which may lead

us nearer to the truth of the whole matter. He writes on pages 2 and 21 to the effect that some so-called "primitive" ideas are not primitive at all in the sense of being ancient but instead are often crude adaptations and borrowings from a higher neighboring culture, often of a relatively late date. This of course is a fact well recognized among critical students and it is one which works havoc with our chronological schemes and in so doing shows us the inevitable danger of intensive and at the same time geographically circumscribed research. Frankly, to one contemplating the archaeology of western Europe from the opposite side of the Atlantic that science has for some four or five years presented several very astonishing inconsistencies. But let Prof. Smith speak.

After warning us of the "confusing chronological implications" in our present use of the terms Paleolithic and Neolithic he urges what he calls "the still more fundamental objection" to these and other definitions, viz., "that the great cultural break in western Europe itself (and even in its flint work) did not fall between the so-called Paleolithic and Neolithic Ages, but between the Lower and Upper Paleolithic periods (pp. 18-29). There is," he continues, "a much closer kinship between the flint-work of the so-called Upper Paleolithic and Neolithic Ages than there is between the former and that of the Lower Paleolithic period" (p. 18). This is a sweeping assertion but one which, in view of the substitution of racial types supposed to have occurred at this time, might well have been expected to be true. Nevertheless, if we are to accept the chronology as determined for France and northern Spain, the reviewer finds it impossible to agree unequivocally. Certainly, the Aurignacian industry is characterized by the seemingly sudden appearance of a whole series of new types in the way of both bone and flint implements; but traces of bone implements do occur in the Mousterian deposits (the reviewer has removed them with his own hands at Castillo) and the Aurignacian flint technique, at least in certain of its phases, is identical with that of the Mousterian epoch. In fact, if our Paleolithic data for western Europe have been correctly and completely presented, then, from the point of view of the flint-working technique, the transition from Mousterian to Aurignacian offers fewer difficulties than that of any other shift, except perhaps the one from Chellean to Acheulean. Thus really important modifications are involved in the transitions from Acheulean to Mousterian, from Aurignacian to Solutrean, and from Solutrean to Magdalenian—all quite as radical in their way as the transition from Magdalenian or Azilian to Neolithic.

Stating the condition in another way, the whole Paleolithic group

series (excepting the Acheulean) have until very recently appeared like so many distinct, successive phenomena not derivable the one from the other. It has looked as if possibly some of the transitional facies did not exist in western Europe; that in other words, these successive inventions were perfected elsewhere—in Asia or Africa, perhaps—and that they reached the Pyrenean foothill country merely as so many successive cultural waves. Thus conceived, the separate group series, in conformity to what seems to be Prof. Smith's general theory, might have originated in one and the same locality; or, what is just as probable, they might have originated each in its own separate locality; or finally, as seems even more likely, some of them might have been re-originated at several different times and places. In short the general outlook with reference to our Paleolithic culture problem has been not unlike that which the outsider still seems to see in physical anthropology and in fact throughout the whole range of evolutionary biology. That is, we have had a geologically or stratigraphically determined time series but no one has been able until recently to demonstrate anything like genetic relationships. Thus to cite an example from physical anthropology our author himself (pp. 16 and 23) considers it doubtful whether *Homo neanderthalensis* was in the direct ancestral line of modern man and with reference to the contemporary implement series he says, on the latter page, that there is nothing to suggest the evolution of one type from another as having taken place in western Europe. The whole is to him a discrete series of phenomena. To all this, however, it was always possible to reply either that the representative transitional stations had not been located or else that the data at hand had not been critically handled. But at the present moment that subterfuge is hardly necessary. The French archaeologists, particularly Breuil, Capitan and Obermaier, have all in recent years made some headway towards bridging the gaps by showing, for example, that certain Solutrean features were clearly foreshadowed in the Aurignacian industry and that certain Acheulean features held over into the Mousterian, etc. It is still possible perhaps to agree with Prof. Smith that our Paleolithic industrial series were most of them invented beyond the confines of western Europe but the transition from one to another even here is not so abrupt as we formerly supposed.

But our disagreement with Prof. Smith is only partial. He administers a well-directed blow to those mostly European archaeologists whose evolutionary ideas have led them to take for granted that the cultural history of France is the natural and necessary cultural history of the world at large. That this assumption is unfounded we have already

suggested in our reference to America but as the author himself points out (p. 19), it can be demonstrated without going beyond the confines of Europe. Thus, as he puts it, "the Mediterranean lands as a whole passed directly from the Aurignacian stage to the Neolithic," the Solutrean and Magdalenian industries never having made their way for instance into Italy and southern Spain. In the same way it now seems timely to add, there are probably other sections of the Old World in which the Solutrean stage passed directly over into the Neolithic, without the interposition of the Magdalenian and Azilian phases. And this brings us to the crux.

Prof. Smith has not told the whole story and besides, his argument has a recoil, as it were. What he said about the irregularity in geographical distribution of our western European culture phases and consequently about the "confusing chronological implications" is doubtless true. But why accept the chronological series localized in France in preference to those of Italy and those of Austria? Stratigraphy is not the only criterion of chronology; the history of culture involves technological considerations which cannot properly be ignored and which in this case appear to show the way out of the difficulty. Thus, however discrete and unrelated may seem the industrial phenomena making up the French Paleolithic series as determined by stratigraphic methods, from the point of view of technique alone there is, I am convinced, something both "natural" and "necessary" in the scheme, at least up to a certain point. That is to say, the methods followed in the production of the successive series of implements do depend the one upon the other and therefore do appear in some measure to be achieved by those "blind forces of an arbitrary and inevitable process of evolution" which Prof. Smith so repeatedly and so vehemently seeks to expose and to discredit (see, *e. g.*, pp. 19, 28, 34, 47, and 49). For just as in the study of human embryology we imagine we see passing in review the main transformation making up the history of the phylum just so in the production of a Solutrean blade the artisan is obliged to repeat the main technical steps of the preceding Paleolithic Age. During the rough reduction of his flint nodule, by means of flaking, he must employ what is essentially the Chellean method and during the finishing process, by means of chipping, must employ what is nothing more nor less than a Mousterian trick brought to perfection. In other words, if we view the Paleolithic industries in the large we see only two great technical steps—the Chellean and the Mousterian; the Acheulean is merely an advance or an improvement on the former as the Aurignacian and Solutrean are successive improvements

on the latter. For this reason—without for a moment calling in question the validity of Paleolithic stratigraphy as determined for western Europe—it seems to the reviewer self-evident that the Solutrean (not the Magdalenian or the Azilian) flint industry is the true and only possible transitional phase connecting the Paleolithic and Neolithic ages. In fact the Solutrean technique as such remains a phase of Neolithic culture to the present day. Stated a little differently, the Solutrean flint industry is the technological outcome and consummation of Paleolithic workmanship: beyond it there was nothing left to do but to fashion implements by pecking and grinding. The latter process of reduction was known already, having been used on bone implements all through Aurignacian times. Therefore, seeing how difficult it has been even stratigraphically to bridge the gap between the Magdalenian and Neolithic cultures by way of the Azilian-Tardenoisian phases, why not recognize at once that technologically it can not be done; that the Magdalenian and Azilian-Tardenoisian stages, as they appear in France, are merely survivals or successive continuations of the Aurignacian which later was temporarily obscured by the incursion of a Solutrean culture wave from some outside point of origin. Or we may explain the situation in another way. There is some reason for believing that the Solutrean technique was actually invented or perfected in the Pyrenean region and that although it did not at once gain general acceptance in western Europe it spread thence eastward over Asia and ultimately into both Africa and America. Meanwhile those who had failed to take up with the Solutrean technique went off on an impossible line of specialization and when they had achieved almost complete extinction with their Azilian-Tardenoisian efforts they were at last engulfed in a return wave of the full Neolithic. But, wherever the Solutrean center of origin, whether in France or in the region of the Black Sea, as suggested by Prof. Smith (p. 19), its center of culmination lies probably somewhere to the east, perhaps in Asia. Some day we shall discover it and when we do we shall doubtless at the same time find the commencement of the so-called Neolithic technique. For an archeologist to predict this seems no more hazardous than for the astronomers to have predicted the position of a missing planet.

With this we shall have to close. We have considered only the two main points to Prof. Smith's paper and while the discussion has been drawn out at some length, it is not, I trust, out of proportion with the importance of the subject. The reviewer has attained neither the years nor the familiarity with the facts of the subject to permit his claiming a

matured judgment; but he is prepared to go a long ways with the writer though, most emphatically, not all the way. But whatever the faults of the paper or of the review, it seems clear that we are much in need of this type of study or otherwise we shall soon be swamped in a multiplicity of details.

N. C. NELSON

NORTH AMERICA

Teton Sioux Music. FRANCES DENSMORE. (Bureau of American Ethnology, Bulletin 61, pages 1-561, plates 1-82.) Washington, 1918.

In this volume Miss Densmore presents and analyzes 240 songs, and describes the ceremonies and occasions on which they are sung. Combined with her Chippewa music in Bulletins 45 and 53, this makes a total of 600 songs which she has rendered available from the two tribes. The volume of this material renders her work a most important contribution in a division of ethnology which is still in its infancy.

Fault will probably be found in some quarters with Miss Densmore's transcriptions on the ground that she has used ordinary musical notation with only an occasional indication of pitch deviation. It is to be hoped that she will not allow such criticism to disturb her. It is no doubt important that some study of primitive music be made with a finer determination of pitch values than the current musical notation allows. But until we know more of the deviations of pitch in our own vocal music, it would be pedantic to insist that a superior standard of discrimination be applied in primitive songs.

On the other hand, the author's treatment of tonality seems open to more valid objection. In assuming the tonality of each song she obviously predetermines its scale and therefore the scales of Sioux music in general. By her non-observance of this fact, Miss Densmore seems to have vitiated entirely the value of her tables 1, 2, 3, 6, 7, 8, and 14 both in her analyses of the Sioux songs (page 12) and the comparison with Chippewa (page 26). It is true that the question of tonality and scale in primitive music is difficult, and that just because the feeling for tonality is obviously less rigid than among ourselves, and the scale somewhat looser, any method of attack is open to a certain arbitrariness. At the same time there is a definite problem, and to take our own system as the point of departure precludes any possibility of determining the native system, however vague this may be. The author's tabulations would certainly have shown more if she had attempted at the outset to ascertain the tone which in native feeling seemed in each song to correspond most closely to our tonic. This would probably be a tone brought into